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DISCUSSION

CAUSE AND GROUND

MR. H. S. SHELTON, in number 10 of the current volume of this JOURNAL, incidentally criticises my treatment of "Cause and Ground."[1]

The criticism amounts to this, I think, that the conception of cause is identified by me with that of ground in such a way as to make it valueless for practise, while the conception of ground itself is in a sphere where scientific investigation has no meaning. I should be sorry for your readers to think the former criticism wholly relevant and the latter wholly true and I should wish to explain what a theory like mine tries to do, and how far I still maintain that it has been successful in doing it.

1. As to value for scientific practise. The purpose of a general work on logic is emphatically not to teach scientific men their business. It is a theory of a whole subject, of which their work is only a part; and its primary aim is to put the whole subject in its natural order in a single view. For this purpose, methods processes and ideas must be put in their place; and may often lose the prominence which they claim in the eyes of workers in some special part of the field of knowledge. But to put them in their place is not to reject them; and although one does not expect to teach scientific men their business, one would be sorry to think either that one was wholly out of harmony with the spirit of their procedure, or that absolutely no light was thrown upon its nature, no scientific advance indicated or corroborated, by the juxtaposition and explanation which one had attempted.

In dealing with the idea of cause[2] I have tried to give reasons for the difficulty and evasiveness of the conception and to explain it as marking a stage in a continuous process of inference, in the higher phases of which it is not applicable. And in quite another place[3] I have tried to trace this continuous process itself—the inductive process from perception upwards, and to show that it does equally well without using at all the terminology or the elementary and popular idea of cause and effect. And here I had hoped, and am still of opinion, that I was at least on parallel lines with and had in some degree anticipated a very powerful movement in the ranks of the most recent workers in the field of exact natural knowledge, who "openly propose to expel the word 'cause' from the language of science."[4]

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[1] See my "Logic," Vol. I., c. VI.; and also Vol. II., c. IV., "Scientific Induction by Perceptive Analysis," or more shortly in "Essentials of Logic," pp. 164–5.

[2] Vol. I., *l. c.*

[3] Vol. II., *l. c.*

But in thus "theorizing" the ordinary conception of cause and effect, I was not suggesting in any way that it is not of value for the working procedure of certain sciences at certain stages. Chemistry, I presume, from which Mr. Shelton draws his instance of argon, is not in a position to aim at complete explanation ("ground"), and though it has aspirations in that direction, yet practically works in the main by direct perceptive analysis of the qualities of objects, and their effect upon one another. Here the conception of cause and effect in the sense of conditions "producing" a new condition, unaccountably different from themselves, is fully in place. But at this level it is impossible to get the conditions fully relevant to the effect, and the causal relation remains subject to "plurality of causes"; which is in flat contradiction with the principle that the effect must vary with the cause, not to speak of the difficulties attending the priority of cause in time.[5]

Now a contradiction in itself is nothing against a working conception. But any one who wishes to regard a subject as a whole must try at least to soften the contradiction by showing how it leads up to something less contradictory.

2. That is why one was driven, following many suggestions of philosophers, and in accordance with a strong tendency in men of science,[6] to exhibit the idea of cause and effect as concerned with events in time or the interaction of substances as leading up to a conception which would include what was true in it but get rid of some at least of its contradictions. I will explain the nature of this conception a little further, and try to show how many cross lights of thought are reconciled in it.

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[4] Taylor, "Elements of Metaphysics," p. 175. I see that in a footnote, *l. c.*, Professor Taylor objects to the suggestion that the "descriptive" theory was a discovery of idealists. And I should not make the claim as bearing on the detailed practise of science; but I will try to show elements of kinship with it which I think are not negligible.

[5] Mr. Shelton in his example does not show his hand on this point. But surely the current idea of cause and effect, if you are going to use it, ties you down to events successive in time.

[6] I suppose I took the idea of "ground" from Hegel, observing that it agreed with points which I shall notice in Plato, and that Mill's notion of the "sum" of conditions when worked out pointed in the same direction, while Clifford roundly said ("Lectures and Essays," I., 150) that cause is simply one word for understanding all about a sequence of events. I did not, I fear, know of the "descriptive" movement, and was glad to be confirmed by it.

I understand my critic to allege it as a fault of this conception that an experience referred to its ground is one which can not be repeated.

And first, this implies that the interest and value of inductive science demands and is addressed to events which are repeated.

But strictly, I think, this is untrue. If the alleged principle of induction "same cause, same effect" were taken as its principle in bitter earnest, there could be no inference and no discovery. The interest of inductive science is surely in getting from the old knowledge to the new, and therefore in interpreting known laws in relation to new suggestions as guide to novel facts. Inductive theory is apt to leave out this side of the matter because it can not be reduced to rule. But all the same, it is where the real stress of induction lies. Professor Stout's "proportional systems" are the nearest thing, perhaps, to what is wanted at this part of the theory.[7] If an occurrence were strictly repeated, without novelty of conditions, its inclusion in an induction would have no value nor interest.

But waiving the above, which might be termed a purist view, is it true that an occurrence stated with reference to its real ground can not be repeated in the sense in which ordinary occurrences are repeated under new conditions?

When this assertion is specially made about an occurrence known in its ground it can only mean that we then *ex hypothesi* know so much about it as to differentiate it from any possible future occurrence. That is to say, our knowledge in the case supposed represents the reality of nature—the complexity of the situation—very much more adequately than when we know it as cause and effect qua separate things or events. If so, surely, for the real inductive interest, which lies in proceeding to new knowledge by disentangling new sequences, the analyzed ground is much more helpful than the mere meaning of a concrete cause whose nature and action we are unable to analyze. In a word "ground" means completer knowledge, and completer knowledge is more practically helpful than less complete.

My critic seems to assume that a reciprocal judgment can not have its content developed and must amount to a tautology. But why? I shall show directly that even as a tautology it may be a very valuable protest against errors which frequently occur in practise. But apart from this, the whole content of a science may be involved in a good definition, which is certainly always a reciprocal judgment.

In a particular case, a given science, which can pronounce on a de facto cause, may very well be unable to supply the detail required for a developed statement of ground. But that is its own lookout. No one doubts that if it could do so it would be better off than it is. Such knowledge is the very ideal on which chemistry is generally understood to have fixed its eyes. If you could say the ultimate law or nature of matter is such, and argon is such and such a modification of it, and its properties flow from this its nature, and among others its gravity, by which we first empirically discovered its existence; that would be a reciprocal judgment, and surely would be an advance towards what knowledge aspires to be. Why not explain a protective resemblance by natural selection as operative on a creature through its special environment? What is there tautologous in this?

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[7] See also my Vol. II., p. 174, on generalization by inclusion in a system as opposed to generalization by an abstract class-name.

But, I will say, tautology is not a bad thing, if it reminds us that an explanation must not leave out the matter to be explained. After all, a thing can only be explained by what includes it. "I can not understand," Plato makes Socrates say, in effect, with a large element of humor and irony, "I can not understand all those cunning causes that people speak of; but I do believe that I am safe in sticking to this, that it is beauty by which things are beautiful." This passage reads like a prophetic criticism of esthetic theories. Suggested explanations of beauty such as pleasure, realism, idealism, pure form, pure feeling, vanish like shadows before such a criterion. Within an explanation, you must retain the thing to be explained.

In ground, you are sure of this. The "effect" has become a case inside a system. In cause and effect you can not be quite sure. The relevancy of cause to effect is incomplete. You do not really know what, in your cause, is causal, and therefore what in it guarantees the effect, because, *ex hypothesi,* effect is outside cause.

I cite another defence of apparent tautology which puts the matter strikingly.

> "If the case is taken as the instance of a rule, even that by itself is *some* explanation. I know it has been said, and by those whom I respect, that we have nothing here but bare tautology; that it is frivolous to tell me that this bottle breaks *because* all bottles break. But I confess that I never could see the *bare* tautology. For the particular nature of our one bottle is in this way connected with a general law. It does not break because it is a black bottle, or a bottle made by an infidel on a Sunday, but because it possesses an unstated quality common to other bottles. And this quality *is* a reason why it breaks. The explanation of course does not satisfy our desires, since we want to make the quality explicit; but, so far as it goes, it does give us some principle, and it can not fairly be condemned as tautologous."[8]

Extremes meet; and while I make no claim to have anticipated the descriptive theorists on scientific ground (the suggestion which

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[8] Bradley, "Principles of Logic," p. 49.

Professor Taylor rejects was not of my making), I do most certainly hold that Plato and Hegel and Mr. Bradley, followed at an enormous interval by myself, were driving at the same point which the "descriptive" movement has worked out on purely scientific soil. And this point is that cause and effect are arbitrary distinctions drawn on the surface of a continuous system or process. And what you must do, if science is to advance towards its aim, is to get this process or system expressed in the simplest terms, showing any effects which may interest you as cases included in it.

At all events, a logical theory has to deal with all these ideas and experiences and bring them together according to their values and interdependence. But in doing so, it does not in the least imply that men of science ought to use other ideas than they do use at particular points in their work. I may add that in my judgment this view of the nature of working ideas according to which they are dictated by the purposes for which they are needed, gives pragmatism all it ought to have, and is, of course a much older view than pragmatism. I know no justification for calling logic which takes such a view—Hegel's logic for example—"intellectualist."

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REVIEWS AND ABSTRACTS OF LITERATURE

Die Taktile Schätzung von ausgefüllten und leeren Strecken. Reprinted from Archiv für Gesamte Psychologie, Bd. XVI., Heft 3 and 4. HELEN DODD COOK. Pp. 130.

The importance of method and procedure and their relation to results in experimental studies is nowhere more apparent than in the field of space perception. Time and again quantitative results which were purely a function of method have been generalized into qualitative principles of the field under investigation. Thus contradictory differences between active and passive movement may be traced to the operation of such secondary criteria as the illusion of impact. Controversy over the absolute "indifference point" in various sense fields depends in large measure on the range of magnitudes employed in different investigations, and many studies of visual interpretation are inadequate because the results are conditioned by the particular range within which the observations were made. In her recent study of the tactual estimation of filled and empty extents Dr. Helen Dodd Cook has admirably demonstrated the rôle of method and procedure in the field of tactual space perception.

The study takes its point of departure from the discordant results of Rieber, Parish and others in their experiments on the familiar illusion of open and filled space, and was begun in an attempt to reconcile these differences. In the course of the experiments other interesting results